CRITERIA FOR SCIENTIFIC CHOICE

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I

As science grows, its demands on our society's resources grow. It seems inevitable that science's demands will eventually be limited by what society can allocate to it. We shall then have to make choices. These choices are of two kinds. We shall have to choose among different, often incommensurable, fields of science—between, for example, high-energy physics and oceanography or between molecular biology and science of metals. We shall also have to choose among the different institutions that receive support for science from the government—among universities, governmental laboratories and industry. The first choice I call scientific choice; the second, institutional choice. My purpose is to suggest criteria for making scientific choices—to formulate a scale of values which might help establish priorities among scientific fields whose only common characteristic is that they all derive support from the government.

Choices of this sort are made at every level both in science and in government. The individual scientist must decide what science to do, what not to do: the totality of such judgments makes up his scientific taste. The research director must choose which projects to push, which to kill. The government administrator must decide not only which efforts to support; he must also decide whether to do a piece of work in a university, a national laboratory, or an industrial laboratory. The sum of such separate decisions determines our policy as a whole. I shall be concerned mainly with the broadest scientific choices: how should government decide between very large fields of science, particularly between different branches of basic science? The equally important question of how government should allocate its support for basic research among industry, governmental laboratories, and universities will not be discussed here.

II

Most of us like to be loved; we hate to make choices, since a real choice alienates the party that loses. If one is rich—more accurately, if one is growing richer—choices can be avoided. Every administrator knows that his job is obviously unpleasant only when his budget has been cut. Thus the urgency for making scientific or institutional choices has in the main been

ignored both in the United States and elsewhere because the science budget has been expanding so rapidly: the United States government spent $1,600,000,000 on research and development in 1950, $9,000,000,000 in 1960, $14,000,000,000 (including space) in 1962.

Though almost all agree that choices will eventually have to be made, some well-informed observers insist that the time for making the choices is far in the future. Their arguments against making explicit choices have several main threads. Perhaps most central is the argument that since we do not make explicit choices about anything else, there is no reason why we should make them in science. Since we do not explicitly choose between support for farm prices and support for schools, or between highways and foreign aid, why should we single out science as the guinea pig for trying to make choices? The total public activity of our society has always resulted from countervailing pressures, exerted by various groups representing professional specialities, or local interests, or concern for the public interest. The combination that emerges as our Federal budget is not arrived at by the systematic application of a set of criteria: even the highest level of authority, in the United States, the President, who must weigh conflicting interests in the scale of the public interest, is limited in the degree to which he can impose an overall judgment by the sheer size of the budget if by nothing else. But because we have always arrived at an allocation by the free play of countervailing pressures this does not mean that such free interplay is the best or the only way to make choices. In any case, even if our choices remain largely implicit rather than explicit, they will be more reasonable if persons at every level, representing every pressure group, try to understand the larger issues and try to mitigate sectional self-interest with concern for broader issues. The idea of conflicting and biased claims being adjudicated at one fell swoop by an all-knowing supreme tribunal is a myth. It is much better that the choices be decentralised and that they reflect the concern for the larger interest. For this reason alone philosophic debate on the problems of scientific choice should lead to a more rational allocation of our resources.

A second thread in the argument of those who refuse to face the problem of scientific choice is that we waste so much on trivialities—on smoking, on advertising, on gambling—that it is silly to worry about expenditures of the same scale on what is obviously a more useful social objective—the increase of scientific knowledge. A variant of this argument is that with so much unused steel capacity or so many unemployed, we cannot rightly argue that we cannot afford a big cyclotron or a large manned-space venture.

Against these arguments we would present the following considerations on behalf of a rational scientific policy. At any given instant, only a certain fraction of our society's resources goes to science. To insist or imply that

the *summum bonum* of our society is the pursuit of science and that therefore all other activities of the society are secondary to science—that unused capacity in the steel mills should go to ' Big Science ' rather than a large-scale housing programme—is a view that might appeal strongly to the scientific community. It is hardly likely to appeal so strongly to the much larger part of society that elects the members of the legislature, and to whom, in all probability, good houses are more important than good science. Thus, as a practical matter we cannot really evade the problem of scientific choice. If those actively engaged in science do not make choices, they will be made anyhow by the Congressional Appropriations Committees and by the Bureau of the Budget, or corresponding bodies in other governments. Moreover, and perhaps more immediately, even if we are not limited by money, we shall be limited by the availability of truly competent men. There is already evidence that our ratio of money to men in science is too high, and that in some parts of science we have gone further more quickly than the number of really competent men can justify.

III

Our scientific and governmental communities have evolved institutional and other devices for coping with broad issues of scientific choice. The most important institutional device in the United States is the President's Science Advisory Committee, with its panels and its staff in the Office of Science and Technology. This body and its panels help the Bureau of the Budget to decide what is to be supported and what is not to be supported. The panel system, however, suffers from a serious weakness. Panels usually consist of specialised experts who inevitably share the same enthusiasms and passions. To the expert in oceanography or in high energy physics, nothing seems quite as important as oceanography or high energy physics. The panel, when recommending a programme in a field in which all its members are interested, invariably argues for better treatment of the field—more money, more people, more training. The panel system is weak insofar as judge, jury, plaintiff and defendant are usually one and the same.

The panel is able to judge how competently a proposed piece of research is likely to be carried out: its members are all experts and are likely to know who are the good research workers in the field. But just because the panel is composed of experts, who hold parochial viewpoints, the panel is much less able to place the proposal in a broader perspective and to say whether the research proposal is of much interest to the rest of science. We can answer the question ' how ' within a given frame of reference; it is impossible to answer ' why ' within the same frame of reference. It would therefore seem that the panel system could be improved if representatives, not

only of the field being judged but also representatives of neighbouring fields, sat on every panel judging the merits of a research proposal. A panel judging high energy physics should have some people from low energy physics; a panel judging low energy physics should have some people from nuclear energy; a panel judging nuclear energy should have some people from conventional energy; and so on. I should think that advice from panels so constituted would be tempered by concern for larger issues; in particular, the support of a proposed research project would be viewed from the larger perspective of the relevance of that research to the rest of science.

In addition to panels or the bodies like the President's Science Advisory Committee as organisational instruments for making choices, the scientific community has evolved an empirical method for establishing scientific priorities, that is, for deciding what is important in science and what is not important. This is the scientific literature. The process of self-criticism, which is integral to the literature of science, is one of the most characteristic features of science. Nonsense is weeded out and held up to ridicule in the literature, whereas what is worthwhile receives much sympathetic attention. This process of self-criticism embodied in the literature, though implicit, is nonetheless real and highly significant. The existence of a healthy, viable scientific literature in itself helps assure society that the science it supports is valid and deserving of support. This is a most important, though little recognised, social function of the scientific literature.

As an arbiter of scientific taste and validity, scientific literature is beset with two difficulties. First, because of the information explosion, the literature is not read nearly as carefully as it used to be. Nonsense is not so generally recognised as such, and the standards of self-criticism, which are so necessary if the scientific literature is to serve as the arbiter of scientific taste, are inevitably looser than they once were.

Second, the scientific literature in a given field tends to form a closed universe; workers in a field, when they criticise each other, tend to adopt the same unstated assumptions. A referee of a scientific paper asks whether the paper conforms to the rules of the scientific community to which both referee and author belong, not whether the rules themselves are valid. So to speak, the editors and authors of a journal in a narrowly specialised field are all tainted with the same poison. As Einstein said, 'Eigener Dreck stinkt nicht.'[1]

Can a true art of scientific criticism be developed, *i.e.*, can one properly criticise a field of science beyond the kind of criticism that is inherent in the literature of the field? Mortimer Taube in *Computers and Common*

[1] As quoted by Dyson, Freeman J., in a review of Sweber, S.S., *Mesons and Fields*, in *Physics Today*, IX (May, 1956), pp. 32–34.

Sense[2] insists that such scientific criticism is a useful undertaking, and that, by viewing a field from a somewhat detached point of view, it is possible to criticise a field meaningfully, even to the point of calling the whole activity fraudulent, as he does in the case of non-numerical uses of computers. I happen to believe that Taube does not make a convincing case in respect to certain non-numerical uses of computers, such as language translation. Yet I have sympathy for Dr. Taube's aims—that, with science taking so much of the public's money, we must countenance, even encourage, discussion of the relative validity and worthwhileness of the science which society supports.

IV

I believe that criteria for scientific choice can be identified. In fact, several such criteria already exist; the main task is to make them more explicit. The criteria can be divided into two kinds: internal criteria and external criteria. Internal criteria are generated within the scientific field itself and answer the question: How well is the science done? External criteria are generated outside the scientific field and answer the question: Why pursue this particular science? Though both are important, I think the external criteria are the more important.

Two internal criteria can be easily identified: (1) Is the field ready for exploitation? (2) Are the scientists in the field really competent? Both these questions are answerable only by experts who know the field in question intimately, and who know the people personally. These criteria are therefore the ones most often applied when a panel decides on a research grant: in fact, the primary question in deciding whether to provide governmental support for a scientist is usually: How good is he?

I believe, however, that it is not tenable to base our judgments entirely on internal criteria. As I have said, we scientists like to believe that the pursuit of science as such is society's highest good, but this view cannot be taken for granted. For example, we now suffer a serious shortage of medical practitioners, probably to some extent because many bright young men who would formerly have gone into medical practice now go into biological research; government support is generally available for post-graduate study leading to the Ph.D. but not for study leading to the medical degree. It is by no means self-evident that society gains from more biological research and less medical practice. Society does not *a priori* owe the scientist, even the good scientist, support any more than it owes the artist or the writer or the musician support. Science must seek its support from society on grounds other than that the science is carried out competently and that it is ready for

[2] (New York: Columbia University Press, 1961).

exploitation; scientists cannot expect society to support science because scientists find it an enchanting diversion. Thus, in seeking justification for the support of science, we are led inevitably to consider external criteria for the validity of science—criteria external to science, or to a given field of science.

V

Three external criteria can be recognised: technological merit, scientific merit and social merit. The first is fairly obvious: once we have decided, one way or another, that a certain technological end is worthwhile, we must support the scientific research necessary to achieve that end. Thus, if we have set out to learn how to make breeder reactors, we must first measure painstakingly the neutron yields of the fissile isotopes as a function of energy of the bombarding neutron. As in all such questions of choice, it is not always so easy to decide the technological relevance of a piece of basic research. The technological usefulness of the laser came after, not before, the principle of optical amplification was discovered. But it is my belief that such technological bolts from the scientific blue are the exception, not the rule. Most programmatic basic research can be related fairly directly to a technological end at least crudely if not in detail. The broader question as to whether the technological aim itself is worthwhile must be answered again partly from within technology through answering such questions as: Is the technology ripe for exploitation? Are the people any good? Partly from outside technology by answering the question: Are the social goals attained, if the technology succeeds, themselves worthwhile? Many times these questions are difficult to answer, and sometimes they are answered incorrectly: for example, the United States launched an effort to control thermonuclear energy in 1952 on a rather large scale because it was thought at the time that controlled fusion was much closer at hand than it turned out to be. Nevertheless, despite the fact that we make mistakes, technological aims are customarily scrutinised much more closely than are scientific aims; at least we have more practice discussing technological merit than we do scientific merit.

VI

The criteria of scientific merit and social merit are much more difficult: scientific merit because we have given little thought to defining scientific merit in the broadest sense, social merit because it is difficult to define the values of our society. As I have already suggested, the answer to the question: Does this broad field of research have scientific merit? cannot be answered within the field. The idea that the scientific merit of a field can be judged better from the vantage point of the scientific fields in which it is

embedded than from the point of view of the field itself is implicit in the following quotation from the late John von Neumann: ' As a mathematical discipline travels far from its empirical source, or still more, if it is a second and third generation only indirectly inspired by ideas coming from reality, it is beset with very grave dangers. It becomes more and more pure aestheticising, more and more purely *l'art pour l'art*. This need not be bad if the field is surrounded by correlated subjects which still have closer empirical connections or if the discipline is under the influence of men with an exceptionally well-developed taste. But there is a grave danger that the subject will develop along the line of least resistance, that the stream, so far from its source, will separate into a multitude of insignificant branches, and that the discipline will become a disorganised mass of details and complexities. In other words, at a great distance from its empirical source, or after much " abstract " inbreeding, a mathematical subject is in danger of degeneration. At the inception the style is usually classical; when it shows signs of becoming baroque, then the danger signal is up.' [3]

I believe there are any number of examples to show that von Neumann's observation about mathematics can be extended to the empirical sciences. *Empirical* basic sciences which move too far from the neighbouring sciences in which they are embedded tend to become ' baroque '. Relevance to neighbouring fields of science is, therefore, a valid measure of the scientific merit of a field of basic science. In so far as our aim is to increase our grasp and understanding of the universe, we must recognise that some areas of basic science do more to round out the whole picture than do others. A field in which lack of knowledge is a bottleneck to the understanding of other fields deserves more support than a field which is isolated from other fields. This is only another way of saying that, ideally, science is a unified structure and that scientists, in adding to the structure, ought always to strengthen its unity. Thus, the original motivation for much of high-energy physics is to be sought in its elucidation of low-energy physics, or the strongest and most exciting motivation for measuring the neutron capture cross sections of the elements lies in the elucidation of the cosmic origin of the elements. Moreover, the discoveries which are acknowledged to be the most important scientifically, have the quality of bearing strongly on the scientific disciplines around them. For example, the discovery of X-rays was important partly because it extended the electromagnetic spectrum but, much more, because it enabled us to see so much that we had been unable to see. The word ' fundamental ' in basic science, which is often used as a synonym for ' important ', can be partly paraphrased into ' relevance to neighbouring areas

[3] Heywood, R. B. (ed.), *The Works of the Mind* (University of Chicago Press, 1947), p. 196.

of science'. I would therefore sharpen the criterion of scientific merit by proposing that, other things being equal, *that field has the most scientific merit which contributes most heavily to and illuminates most brightly its neighbouring scientific disciplines.* This is the justification for my previous suggestion about making it socially acceptable for people in *related* fields to offer opinions on the scientific merit of work in a given field. In a sense, what I am trying to do is to extend to basic research a practice that is customary in applied science: a project director trying to get a reactor built on time is expected to judge the usefulness of component development and fundamental research which bears on his problems. He is not always right; but his opinions are usually useful both to the researcher and to the management disbursing the money.

VII

I turn now to the most controversial criterion of all—social merit or relevance to human welfare and the values of man. Two difficulties face us when we try to clarify the criterion of social merit: first, who is to define the values of man, or even the values of our own society; and second, just as we shall have difficulty deciding whether a proposed research helps other branches of science or technology, so we will have even greater trouble deciding whether a given scientific or technical enterprise indeed furthers our pursuit of social values, even when those values have been identified. With some values we have little trouble: adequate defence, or more food, or less sickness, for example, are rather uncontroversial. Moreover, since such values themselves are relatively easy to describe, we can often guess whether a scientific activity is likely to be relevant, if not actually helpful, in achieving the goal. On the other hand, some social values are much harder to define: perhaps the most difficult is national prestige. How do we measure national prestige? What is meant when we say that a man on the moon enhances our national prestige? Does it enhance our prestige more than, say, discovering a polio vaccine or winning more Nobel Prizes than any other country? Whether or not a given achievement confers prestige probably depends as much on the publicity that accompanies the achievement as it does on its intrinsic value.

Among the most attractive social values that science can help to achieve is international understanding and cooperation. It is a commonplace that the standards and loyalties of science are trans-national. A new element has recently been injected by the advent of scientific research of such costliness that now it is prudent as well as efficient to participate in some form of international cooperation. The very big accelerators are so expensive that international laboratories such as CERN at Geneva are set up to enable

several countries to share costs that are too heavy for them to bear separately. Even if we were not committed to improving international relations we would be impelled to cooperate merely to save money.

Bigness is an advantage rather than a disadvantage if science is to be used as an instrument of international cooperation: a $500,000,000 cooperative scientific venture—such as the proposed 1,000 Bev intercontinental accelerator—is likely to have more impact than a $500,000 Van de Graaff machine. The most expensive of all scientific or quasi-scientific enterprises —the exploration of space—is, from this viewpoint, the best-suited instrument for international cooperation. The exchange between President Kennedy and Chairman Khrushchev concerning possible increased cooperation in space exploration seems to have been well received and, one hopes, will bear ultimate fruit.

VIII

Having set forth these criteria and recognising that judgments are fraught with difficulty, I propose to assess five different scientific and technical fields, in the light of these. The five fields I choose are molecular biology, high-energy physics, nuclear energy, manned-space exploration, and the behavioural sciences. Two of these fields, molecular biology and high-energy physics, are, by any definition, basic sciences; nuclear energy is applied science, the behavioural sciences are a mixture of both applied and basic science. Manned exploration of space, though it requires the tools of science and is regarded in the popular mind as being part of science, has not yet been proved to be more than quasi-scientific, at best. The fields which I choose are incommensurable: how can one measure the merit of behavioural sciences and nuclear energy on the same scale of values? Yet the choices between scientific fields will eventually have to be made whether we like it or not. Criteria for scientific choice will be most useful only if they *can* be applied to seemingly incommensurable situations. The validity of my proposed criteria depends on how well they can serve in comparing fields that are hard to compare.

Of the scientific fields now receiving public support, perhaps the most successful is molecular biology. Hardly a month goes by without a stunning success in molecular biology being reported in the *Proceedings of the National Academy of Sciences*. The most recent has been the cracking by Nierenberg and Ochoa of the code according to which triples of bases determine specific amino acids in the living proteins. Here is a field which rates the highest grades as to its ripeness for exploitation and competence of its workers. It is profoundly important for large stretches of other biological sciences—genetics, cytology, microbiology—and, therefore, according to my

criterion, must be graded A+ for its scientific merit. It also must be given a very high grade in social merit, and probably in technological (that is, medical) merit—more than, say, taxonomy or topology. Molecular biology is the most fundamental of all the biological sciences. With understanding of the manner of transmission of genetic information ought to come the insights necessary for the solution of such problems as cancer, birth defects, and viral diseases. Altogether, molecular biology ought, in my opinion, to receive as much public support as can possibly be pumped into it; since money is not limiting its growth, many more post-graduate students and research fellows in molecular biology ought to be subsidised so that the attack on this frontier can be expanded as rapidly as possible.

The second field is high-energy physics. This field of endeavour originally sought as its major task to understand the nuclear force. In this it has been only modestly successful; instead, it has opened an undreamed-of subnuclear world of strange particles and hyperons, a world in which mirror images are often reversed. The field has no end of interesting things to do, it knows how to do them, and its people are the best. Yet I would be bold enough to argue that, at least by the criteria which I have set forth—relevance to the sciences in which it is embedded, relevance to human affairs, and relevance to technology—high-energy physics rates poorly. The nuclear forces are not being worked on very directly—the world of subnuclear particles seems to be remote from the rest of the physical sciences. Aside from the brilliant resolution of the τ-particle paradox, which led to the overthrow of the conservation of parity, and the studies of mesic atoms (the latter of which is not done at *ultra*-high energy), I know of few discoveries in ultra-high-energy physics which bear strongly on the rest of science. (This view would have to be altered if machines such as the Argonne Zero Gradient Synchrotron were exploited as very strong, pulsed sources of neutrons for study of neutron cross sections.) As for its bearing on human welfare and on technology, I believe it is essentially nil. These two low grades would not bother me if high-energy physics were cheap. But it is terribly expensive—not so much in money as in highly qualified people, especially those brilliant talents who could contribute so ably to other fields which contribute much more to the rest of science and to humanity than does high-energy physics. On the other hand, if high-energy physics could be made a vehicle for international cooperation—if the much-discussed intercontinental 1,000 Bev accelerator could indeed be built as a joint enterprise between East and West—the expense of high-energy physics would become a virtue, and the enterprise would receive a higher grade in social merit than I would now be willing to assign to it.

Third is nuclear energy. This being largely an applied effort, it is very relevant to human welfare. We now realise that in the residual uranium and thorium of the earth's crust, mankind has an unlimited store of energy—enough to last for millions of years; and that with an effort of only one-tenth of our manned-space effort we could, within ten or fifteen years, develop the reactors which would tap this resource. Only rarely do we see ways of *permanently* satisfying one of man's major needs—in this case energy. In high-conversion ratio nuclear reactors we have such means, and we are close to their achievement. Moreover, we begin to see ways of applying very large reactors of this type to realise another great end, the economic desalination of the ocean. Thus, the time is very ripe for exploitation. Nuclear energy rates so highly in the categories of technical and social merit and timeliness that I believe it deserves strong support, even if it gets very low marks in the other two categories—its personnel and its relationship to the rest of science. Suffice it to say that in my opinion the scientific workers in the field of nuclear energy are good and that nuclear energy in its basic aspects has vast ramifications in other scientific fields.

Next on the list are the behavioural sciences—psychology, sociology, anthropology, economics. The workers are of high quality; the sciences are significantly related to each other, they are deeply germane to every aspect of human existence. In these respects the sciences deserve strong public support. On the other hand, it is not clear to me that the behavioural scientists, on the whole, see clearly how to attack the important problems of their sciences. Fortunately, the total sum involved in behavioural science research is now relatively tiny—as it well must be when what are lacking are deeply fruitful, and generally accepted, points of departure.

Finally, I come to manned-space exploration. The personnel in the programme are competent and dedicated. With respect to ripeness for exploitation, the situation seems to me somewhat unclear. Our 'hardware' is in good shape, and we can expect it to get better—bigger and more reliable boosters, better communication systems, etc. What is not clear is the human being's tolerance of the space environment. I do not believe that either the hazards of radiation or of weightlessness are sufficiently explored yet positively to guarantee success in our future manned-space ventures.

The main objection to spending so much manpower, not to say money, on manned-space exploration is its remoteness from human affairs, not to say the rest of science. In this respect space (the exploration of very large distances) and high-energy physics (the exploration of very small distances) are similar, though high-energy physics has the advantage of greater scientific validity. There are some who argue that the great adventure of man into space is not to be judged as science, but rather as a quasi-scientific

enterprise, justified on the same grounds as those on which we justify other non-scientific national efforts. The weakness of this argument is that space requires many, many scientists and engineers, and these are badly needed for such matters as clarifying our civilian defence posture or, for that matter, working out the technical details of arms control and foreign aid. If space is ruled to be non-scientific, then it must be balanced against other non-scientific expenditures like highways, schools or civil defence. If we do space-research because of prestige, then we should ask whether we get more prestige from a man on the moon than from successful control of the water-logging problem in Pakistan's Indus Valley Basin. If we do space-research because of its military implications, we ought to say so—and perhaps the military justification, at least for developing big boosters, is plausible, as the Soviet experience with rockets makes clear.

IX

The main weight of my argument is that the most valid criteria for assessing scientific fields come from without rather than from within the scientific discipline that is being rated. This does not mean that only those scientific fields deserve priority that have high technical merit or high social merit. Scientific merit is as important as the other two criteria, but, as I have argued, scientific merit must be judged from the vantage point of the scientific fields in which each field is embedded rather than from that of the field itself. If we support science in order to maximise our knowledge of the world around us, then we must give the highest priority to those scientific endeavours that have the most bearing on the rest of science.

The rather extreme view which I have taken presents difficulties in practice. The main trouble is that the bearing that one science has on another science so often is not appreciated until long after the original discoveries have been made. Who was wise enough, at the time Purcell and Bloch first discovered nuclear magnetic resonance, to guess that the method would become an important analytical tool in biochemistry? Or how could one have guessed that Hahn and Strassmann's radiochemical studies would have led to nuclear energy? And indeed, my colleagues in high-energy physics predict that what we learn about the world of strange particles will in an as yet undiscernible way teach us much about the rest of physics, not merely much about strange particles. They beg only for time to prove their point.

To this argument I say first that choices are always hard. It would be far simpler if the problem of scientific choice could be ignored, and possibly in some future millennium it can be. But there is also a more constructive response. The necessity for scientific choice arises in ' Big Science ', not in

'Little Science'. Just as our society supports artists and musicians on a small scale, so I have no objection to—in fact, I strongly favour—our society supporting science that rates zero on all the external criteria, provided it rates well on the internal criteria (ripeness and competence) and provided it is carried on on a relatively small scale. It is only when science really does make serious demands on the resources of our society—when it becomes 'Big Science'—that the question of choice really arises.

At the present time, with our society faced with so much unfinished and very pressing business, science can hardly be considered its major business. For scientists as a class to imply that science can, at this stage in human development, be made the main business of humanity is irresponsible—and, from the scientist's point of view, highly dangerous. It is quite conceivable that our society will tire of devoting so much of its wealth to science, especially if the implied promises held out when big projects are launched do not materialise in anything very useful. I shudder to think what would happen to science in general if our manned-space venture turned out to be a major failure, if it turned out, for example, that man could not withstand the re-entry deceleration forces after a long sojourn in space. It is as much out of a prudent concern for their own survival, as for any loftier motive, that scientists must acquire the habit of scrutinising what they do from a broader point of view than has been their custom. To do less could cause a popular reaction which would greatly damage mankind's most remarkable intellectual attainment—Modern Science—and the scientists who created it and must carry it forward.